

# Aid, Disbursement Delays, and the Real Exchange Rate

*Alexandra Jarotschkin*

*Aart Kraay*

The World Bank  
Development Research Group  
Macroeconomics and Growth Team  
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## Abstract

Aid donors and recipients have long been concerned that aid inflows may lead to an appreciation of the real exchange rate and an associated loss of competitiveness. This paper provides new evidence of the dynamic effects of aid on the real exchange rate, using an identification strategy that exploits the long delays between the approval of aid projects and the subsequent disbursements on

them. These disbursement delays enable the isolation of a source of variation in aid inflows that is uncorrelated with contemporaneous macroeconomic shocks that may drive both aid and the real exchange rate. Using this predetermined component of aid as an instrument, there is little evidence that aid inflows lead to significant real exchange rate appreciations.

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# Aid, Disbursement Delays, and the Real Exchange Rate

Alexandra Jarotschkin

Aart Kraay\*

The World Bank

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\* Corresponding author, 1818 H Street NW, Washington, DC 20433, USA, [akraay@worldbank.org](mailto:akraay@worldbank.org). We are grateful to Oscar Jordà and Luis Servén for helpful comments, and to Nanasamudd Chhim, Evis Rucaj, Shelley Lai Fu, and Ibrahim Levent for their assistance with the Debtor Reporting System data used in this paper. Financial support from the Knowledge for Change Program (KCP) of the World Bank is gratefully acknowledged. The views expressed here are the authors' and do not reflect those of the World Bank, its Executive Directors, or the countries they represent.

## 1. Introduction

A perennial concern among aid donors and recipient country policymakers is that aid inflows may have adverse side effects on recipient countries. One of these is that large aid inflows lead to upward pressure on the real exchange rate, and an associated loss of competitiveness in the export sector, which could in principle undermine the growth benefits of aid. In this paper, we provide new empirical evidence on the short- to medium-run impacts of aid on the real exchange rate, in a large annual panel of 104 developing countries over the period 1979-2010<sup>1</sup>. We use the local projections method suggested by Jordà (2005) to flexibly describe the dynamic response of real exchange rates to aid inflows. Our basic findings are easily summarized: a one percentage point increase in aid as a fraction of GDP leads to a small and statistically-significant real depreciation of about 0.5 percent on impact, which gradually reverses over time to a small, but statistically insignificant, real appreciation of about 0.5 percent after five years. These findings cast doubt on the view that substantial real appreciations are a likely adverse side effect of aid for recipient countries. If anything, they mirror the theoretical ambiguities about the potential effects of aid on the real exchange rate, that we discuss in more detail below.

An important methodological challenge in isolating the impact of aid on the real exchange rate is that aid inflows are not randomly assigned to recipient countries. Rather, fluctuations in aid within countries over time may very well respond to macroeconomic shocks that also matter for the real exchange rate. For example, a macroeconomic crisis might lead to a large depreciation in the nominal exchange rate, and, given some price stickiness, also the real exchange rate. If the country experiencing the crisis also receives aid inflows in response to the crisis, this would create a spurious correlation between real depreciations and aid inflows. Similarly, a period of poor macroeconomic policy performance leading to high inflation might lead to a real appreciation, and at the same time cause donors who tie aid to policy performance to reduce their aid.

We address these endogeneity concerns by constructing a predetermined component of aid associated with past approvals of loans from official creditors to developing country governments, that can be used as an instrument for overall aid. Official creditors include multilateral aid agencies such as the World Bank, African Development Bank, and Asian Development Bank, as well as major bilateral aid agencies. Disbursements on concessional loans from official creditors are an important form of aid to

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<sup>1</sup> The main data constraint is the multilateral trade-weighted real exchange rate (IMF) that is only available from 1979 whereas the other core variables are available from 1970 onwards.

developing country governments. We take advantage of the fact that disbursements on such loans typically are spread out over several years following the approval of the loan. This is because these loans often finance specific multiyear development spending projects in recipient countries, and official creditors typically tie their disbursements to stages of project implementation. This in turn implies that the majority of disbursements in a given country-year are associated with aid projects approved in many previous years, and prior to the realization of contemporaneous macroeconomic shocks.

The delays between aid approvals and eventual disbursements can be used to isolate a predetermined component of aid. Following Kraay (2012b), we combine actual loan approvals with typical loan disbursement profiles to construct a synthetic measure of disbursements on loans from official creditors to a given country in a given year, that reflects only loan approvals made in previous years. Under the plausible identifying assumption that official creditors' loan approval decisions do not anticipate future macroeconomic shocks, this measure is a valid instrument in a regression of the real exchange rate on aid.

We clearly are not the first to study the effects of aid on real exchange rates. As we discuss in more detail in the following section, many studies have offered theoretical qualifications to the basic mechanism, pointing out that the effect of aid on the real exchange rate is theoretically ambiguous, and depends on how aid is spent, on the structure of the aid-receiving economy, and on the nature of the macroeconomic policy response to the aid inflow. Several empirical papers, also summarized below, have deployed a range of techniques in different samples of developing countries to attempt to tease out the relationships between aid and the real exchange rate in the data. Our main contribution to this literature is to propose a more careful strategy for isolating a predetermined component of aid flows that can be used to more credibly identify the response of the real exchange rate to aid inflows. In addition, our use of local projections allows a much more flexible approach to characterizing the dynamic response of the real exchange rate to aid inflows.

The rest of this paper proceeds as follows. Section 2 provides more background on the mechanisms linking aid inflows and the real exchange rate, and briefly reviews the existing empirical evidence. Section 3 presents our empirical framework and describes the data sources. Section 4 reports benchmark results, and Sections 5 and 6 extend them in various directions. Section 7 concludes.

## 2. Background and Related Literature

The basic mechanism through which aid inflows may lead to a real appreciation is well-understood. If aid inflows are spent primarily on non-traded goods, this will bid up the relative price of non-tradeables. Since tradeables prices are fixed by global conditions, this implies an increase in the domestic price level and a real exchange rate appreciation. The real appreciation will be larger the more inelastic is the supply of non-traded goods, and the larger is the fraction of the aid inflow that is spent on non-traded goods.

It is important to note that such a real appreciation by itself does not imply that the aid-receiving country is worse off. The country benefits from the initial resource transfer, and the real appreciation simply reflects the transfer of resources from the traded to the non-traded sector. After the aid inflow has been spent, resources shift back to their initial allocation. If, however, one assumes further that there is learning-by-doing or some other production externality in the traded sector, then the temporary shift in production away from tradeables can have a persistent negative effect on productivity in the traded sector. If this effect is strong enough, the country might actually be worse off. Corden and Neary (1982) and van Wijnbergen (1986) provide early theoretical accounts of this basic mechanism, often referred to as the "Dutch Disease" in reference to the decline of the manufacturing sector in the Netherlands following its discovery of offshore gas reserves in the late 1950s. Variants on this idea include Adam (2006) who argues that when domestic financial markets are shallow, firms in the traded sector may not be able to obtain the financing that they need to bridge a period of temporarily lower profits during the real appreciation. More generally, any domestic distortion that makes it costly to shift resources from the traded sector to the non-traded sector will offset the benefit of the initial aid inflow.

Several authors have qualified this basic mechanism in different dimensions. For example, Torvik (2001), Adam (2006), Hussain et. al. (2009), and Temple (2010) all observe that if aid-financed public spending also raises productivity in the non-tradeable sector (for example, by providing needed infrastructure), then this positive supply response will offset the basic mechanism underlying the real appreciation. Adam (2006) also notes that the impact of aid on the real exchange rate will be muted if there is excess capacity in the non-tradeables sector, so that the increase in demand for non-tradeables can be met with little increase in their relative price. Another basic consideration noted by Hussain et. al. (2009) and Adam (2006) is whether aid is spent or not. In principle, aid recipients may simply save

the aid, or use the proceeds of aid to reduce taxes, so that there is no direct aid-induced increase in spending on nontradeables that might contribute to a real overvaluation.

Perhaps reflecting these theoretical ambiguities, empirical evidence on the effects of aid inflows on the real exchange rate is mixed. Referring to the existing evidence at the time, Bulir and Lane (2002) note that it has uncovered "traces" of aid-induced real appreciations. For example, Yano and Nugent (1999) estimate OLS regressions of real exchange rates on aid for 44 aid-dependent countries, for the period 1970-1990. They find that 21 countries display an appreciation and 23 display a depreciation, with the estimated effects in either direction being rarely statistically significant. On the other hand, Elbadawi (1999) estimates OLS regressions of the real exchange rate on aid in a sample of 62 countries between 1970 and 1996, and finds a very small but statistically significant effect: a 35 percent increase in aid increases the real exchange rate by just three percent.

More recent studies also show mixed evidence. Hussain et. al. (2009) present case studies of five African economies experiencing surges in aid inflows, and show that there is no evidence of subsequent real appreciations. On the other hand, Prati, Sahay, and Tressel (2003) estimate the effect of aid on a more sophisticated measure of the real exchange rate which replaces official nominal exchange rates with black market exchange rates. Using dynamic panel estimation techniques, they find a small but statistically significant impact elasticity of the real exchange rate to aid of just four percent, and up to 18 percent after five years. In contrast, Christiansen, Prati, Ricci and Tressel (2009) document a negative long-run relationship between the real exchange rate and aid, suggesting that aid in the long term leads to larger productivity gains in the non-traded sector than in the traded sector. Finally, Rajan and Subramanian (2011) find a positive relationship between a measure of real overvaluation and aid inflows in a cross-sectional regression using decadal averages of the two variables. They also provide evidence that labor-intensive industries (which they equate with tradeables) grow more slowly in countries which receive more aid.

### **3. Empirical Methodology and Data**

We use the local projections method suggested by Jordà (2005) to flexibly document the dynamic response of real exchange rates to aid inflows. We work with annual data on aid inflows and real exchange rates for a large panel of 104 developing countries over the past 32 years. Our starting point is a very parsimonious sequence of local projection regressions:

$$(1) \quad rxr_{i,t+h} = \sum_{s=1}^p \rho_s^h rxr_{i,t-s} + \beta^h aid_{it} + \mu_i^h + \lambda_t^h + \varepsilon_{it}^h$$

where  $rxr_{i,t}$  is the log-level of the real exchange rate;  $aid_{it}$  is foreign aid as a share of GDP;  $\mu_i^h$  and  $\lambda_t^h$  are country and year effects;  $\varepsilon_{it}^h$  is an error term capturing all other sources of variation in the real exchange rate between  $t$  and  $t+h$ ; and  $i$  and  $t$  index countries and years, respectively. The coefficients on aid,  $\beta^h$ , trace out the effect of an increase in aid at time  $t$  on the real exchange rate at time  $t+h$ . Given that aid is measured as a fraction of GDP, and the real exchange rate is measured in logarithms, the estimated coefficients on aid measure the percentage change in the real exchange rate at time  $t+h$  in response to a one percentage point of GDP increase in aid at time  $t$ .

The key insight in Jordà (2005) is that these local projection regressions are a more flexible way of characterizing the dynamic response of variables to a shock than the impulse responses obtained from inverting an autoregressive distributed lag model (or more generally, a vector autoregression). To give a very simple example, suppose that we estimated the following process for the real exchange rate as a function of aid:

$$(2) \quad rxr_{i,t} = \gamma rxr_{i,t-1} + \beta aid_{it} + \mu_i + \varepsilon_{it}$$

This process implies an impulse response of the real exchange to an increase in aid that is of a very particular functional form:

$$(3) \quad \frac{\partial rxr_{i,t+h}}{\partial aid_{it}} = \beta \gamma^h$$

In contrast, the local projection regressions in Equation (1) do not place any restrictions on the shape of the estimated impulse response, and so are much more robust to misspecification of the underlying stochastic process for the real exchange rate. Of course, if the true data generating process is given by Equation (2), then the impulse responses based on Equations (1) and (3) will coincide.

In our context, an additional complication comes from the fact that it does not make sense to think of aid as a strictly exogenous shock in Equation (1). In particular, one might worry that aid inflows into a country respond to various macroeconomic shocks in the recipient country, and that these shocks



also have some direct impact on the real exchange rate. For example, policy reforms that improve productivity in the nontradeables sector might contribute to a real depreciation, and in addition prompt more aid inflows from donors that tie aid to policy performance, implying that  $E[a_{it}\varepsilon_{it}] \neq 0$ . In this case, OLS estimates of the local projections regressions will not deliver a consistent estimate of the effects of aid on the real exchange rate at various horizons.<sup>2</sup>

We address this endogeneity problem using an instrument developed in Kraay (2012b). The instrument exploits the fact that there are long lags between the approval and eventual full disbursement on individual aid projects in developing countries that are financed by concessional loans from official creditors. Such concessional loans are a major vehicle for providing aid to developing countries. They typically finance specific aid projects that take many years to implement, and the disbursements usually are tied to the stages of project implementation, rather than disbursing all at once upon loan approval. These disbursement delays can be used to isolate a component of disbursements in a given country-year that is associated with project and loan approval decisions made in previous years, before contemporaneous macroeconomic shocks are known.

The key identifying assumption is that, while loan approval decisions may respond to current and past macroeconomic shocks, they do not respond to future shocks. If, in addition, loans disbursed as scheduled at the time of approval, then subsequent disbursements on these loans would be uncorrelated with contemporaneous macroeconomic shocks. However, as discussed in more detail in Kraay (2012a,b), this additional assumption is not very plausible, as disbursement decisions on existing loans are made in real time, and may very well respond to contemporaneous shocks. Instead, we rely on a synthetic measure of predicted disbursements, which combines actual loan approvals with "typical" disbursement rates for other loans from the same creditor, in the same region and decade. As a result, the only country-specific variation in predicted disbursements comes from loan approvals from previous years. Conditional on the identifying assumption that these do not respond to future shocks, predicted

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<sup>2</sup> Another potential source of endogeneity is the correlation between the lags of the dependent variable and the unobserved country effect in the error term, which we control for using country fixed effects. This correlation becomes negligible as the time series dimension becomes large. Monte Carlo evidence in Judson and Owen (1999) suggests a bias in the fixed effects estimator of only about 1-3% of the true value of  $\beta$  when  $T = 20$  (their Table 1). While they do report larger biases in the fixed-effects estimator of the coefficient on the lagged dependent variable of 12-17% of the true parameter value, this parameter is not our main interest. In our setting, the median number of time series observations per country is  $T = 28$

disbursements in a given country-year associated with past loan approvals will be uncorrelated with contemporaneous shocks and will thus be a valid instrument for aid.<sup>3</sup>

We therefore estimate the local projection regressions in Equation (1) by two-stage least squares, using predicted disbursements on loans from official creditors, expressed as a share of GDP, as an instrument for aid. Since the set of right-hand-side variables in the local projection regressions is the same for all projection horizons, there is just one first-stage regression of aid on the instrument, lags of the real exchange rate, and of course country and year fixed effects. As we will document in the following sections, predicted disbursements on loans from official creditors has very strong predictive power for actual aid, implying a strong first-stage relationship.

The main aid variable of interest is the standard measure of net official development assistance (ODA) reported by the OECD's Development Assistance Committee. Net ODA consists of disbursements on concessional loans and disbursements on grants, and nets out repayments on past concessional loans. By design, the instrument described above will only have predictive power for the first component of aid, disbursements on concessional loans.<sup>4</sup> However, as we shall see below, despite this limitation the instrument has very strong predictive power for overall aid. Finally, our benchmark measure of the real exchange rate is the IMF's trade-weighted multilateral real exchange rate. This is oriented such that an increase (decrease) corresponds to an appreciation (depreciation). We rebase these so that the real exchange rate index is equal to 100 in 2005 for each country. This implies that only within-country over-time fluctuations in the real exchange rate are economically meaningful, and accordingly in all our empirical specifications that follow we include a full set of country fixed effects.

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<sup>3</sup> The predicted disbursements instrument is constructed as follows. We start with a set of approximately 60,000 loans from official creditors to developing country governments, as recorded in the Debtor Reporting System database of the World Bank. For each loan we have the year of the original commitment, and the schedule of subsequent annual actual disbursements. For each loan we construct a 10-year disbursement profile, i.e. the fraction of the original commitment that is disbursed in year 0, 1, 2, ..., 10, with year zero corresponding to the approval year of the loan. We next sort loans into creditor-decade-region-specific bins and average disbursement profiles across loans within bins. We then apply these typical disbursement profiles to individual loans within each bin, and construct predicted disbursements for each loan, i.e. the disbursements that would have occurred had the loan disbursed at the typical rate for all loans in the same bin. The rationale for this step is that it removes any country-specific source of variation in actual disbursements on previously approved loans, which might respond endogenously to contemporaneous macroeconomic shocks. Finally, we aggregate the predicted disbursements across all loans for each country-year observation, but excluding loans approved in the same year. By construction, the only country-specific variation in this predicted disbursements measure reflects loan approval decisions from previous years, which we assume to be uncorrelated with contemporaneous macroeconomic shocks. For details, see Sections 2 and 3 of Kraay (2012b).

<sup>4</sup> A companion paper, Kraay (2013) extends the methodology in Kraay (2012a,b) to develop an analogous instrument for grant-financed aid.

Table 1 reports summary statistics on aid, the instrument, and the logarithm of the real exchange rate. Net ODA and the instrument are scaled by GDP as defined in Equation (1). In addition, country- and year-specific means are removed before calculating summary statistics, in order to be consistent with the empirical specifications that follow, all of which will also control for a full set of country and year dummies. Summary statistics are shown for three samples of countries. Our core regression sample consists of all country-year observations for which the current and two lagged values of the real exchange rate are available, net ODA is available, and there are at least 15 such annual observations for the country. Overall, this sample consists of 104 countries. The second sample consists of those countries where net ODA as a fraction of GDP is at least 6%, averaging over all annual observations. This sample encompasses 53 countries. The third sample consists of those countries in the full sample that are eligible for borrowing from the International Development Association, the concessional lending arm of the World Bank, and consists of 58 countries.

Within-country fluctuations in the real effective exchange rate and aid are quite volatile in all three samples, with standard deviations ranging from 11 to 12 percent, and from 4 to 6 percent, respectively. In contrast, the instrument based on predicted disbursements is considerably less volatile, at around 1 percent. Fluctuations in aid and the instrument are quite strongly correlated within countries over time, with correlations ranging from 0.37 to 0.39, depending on the sample. The strength of this first-stage relationship will of course be crucial to the success of the identification strategy.

#### **4. Benchmark Results**

Table 2 reports the benchmark estimates of the effect of aid on the real exchange rate using Equation (1). Panel A of Table 2 reports the OLS estimates of the impulse response of the real exchange rate to a one-year, one percent of GDP increase in aid, over a five-year horizon, i.e.  $\beta^h$  for  $h = 0, 1, \dots, 5$ . In each local projection regression, we set the number of lags of the real exchange rate to  $p = 2$ . However, to conserve on space, we report these estimated autoregressive terms only for the  $h = 0$  local projection regression. The three sets of columns correspond to the three different samples: the full sample, the high aid sample, and the IDA sample.

The OLS estimates of the impulse responses are quite similar across the three different samples. On impact, following a one percentage point of GDP increase in aid, there is a small but fairly precisely estimated real depreciation, between 0.23 and 0.26 percent, with standard errors around 0.08 percent.

Over time, there is a gradual appreciation, and after 5 years the real exchange rate is slightly appreciated relative to the initial period when the increase in aid occurred, in the high-aid and IDA samples of countries. However, this small real appreciation is not significantly different from zero.

Panel B of Table 2 reports the 2SLS estimates of the impulse responses. Qualitatively, the pattern that emerges from these is similar to the OLS estimates in Panel A. On impact, there is a small but statistically significant depreciation, that gradually reverses into a small appreciation after five years. The estimated impulse response is steeper, with an initial depreciation of around 0.5 percent following a one percentage point increase in aid as a share of GDP, and ultimately an appreciation of 0.6 to 0.7 percent after five years. As with the OLS estimates, only the initial depreciation is statistically significant at conventional levels. While the 2SLS estimates of the impulse response point to a more pronounced effect of aid on the real exchange rate than do the OLS estimates, the difference between the two is small relative to the estimated standard errors, suggesting that the overall effect of various potential endogeneity biases in the OLS estimates is small.

An important methodological challenge in isolating the impact of aid on the real exchange rate is that aid inflows are not randomly assigned to recipient countries. Rather, fluctuations in aid within countries over time may very well respond to macroeconomic shocks that also matter for the real exchange rate. For example, a macroeconomic crisis might lead to a large depreciation in the nominal exchange rate, and, given some price stickiness, also the real exchange rate. If the country experiencing the crisis also receives aid inflows in response to the crisis, this would create a spurious correlation between real depreciations and aid inflows. Similarly, a period of poor macroeconomic policy performance leading to high inflation might lead to a real appreciation, and at the same time cause donors who tie aid to policy performance to reduce their aid.

While the effects of aid on the real exchange rate are not statistically significant from zero, this is not because they are very imprecisely estimated. The standard errors associated with the 2SLS estimates of the impulse response, although naturally somewhat larger than the OLS estimates, are at most only around 0.5 after five years. This means that our data and methodology are sufficiently informative that a one percent real appreciation after five years in response to a one percentage point of GDP increase in aid would be statistically significantly different from zero. The reasonable precision of the 2SLS estimates is in part due to the strong fit of the first-stage regressions, summarized at the bottom of Table 2. The first stage relationships between net ODA and predicted disbursements on loans from official creditors are very precisely estimated in all three samples, with F-statistics for the excluded

instrument ranging from 49 to 71, far above the Staiger and Stock (1997) rule of thumb of 10. In addition, **Figure 2** provides a visual summary of the first-stage and reduced-form regressions underlying the estimated effects in Table 2, for the full sample of countries. A quick look at this graph confirms that our findings are not driven by a few extreme observations, but rather reflect reasonably systematic patterns in the data.

The validity of the 2SLS estimates of the impulse response depends crucially on the identifying assumption that aid approval decisions in past years are uncorrelated with contemporaneous and future shocks that drive the real exchange rate. There are (at least) two immediate objections to this identifying assumption. The first is that there may be common country-specific trends in aid approvals and the real exchange rate. For example, as a country develops, we might expect both (i) a trend appreciation in the real exchange rate, to the extent that Balassa-Samuleson effects are important, and (ii) a reduction in approval of aid projects as the country's need for foreign aid declines over time. To address this concern, we re-estimated the specifications in Table 2 including country-specific linear time trends. This has virtually no effect on the estimated impulse responses in our benchmark specification, suggesting that this possibility of omitted common trends in the instrument and the real exchange rate is not undermining the validity of the identification strategy.

The second potential objection is that aid-financed projects might crowd in (or crowd out) other forms of government spending. If government spending falls disproportionately on non-tradeables, then these fluctuations in non-aid-financed government spending induced by approvals of aid-financed projects might have direct effects on the real exchange rate, again potentially violating the exclusion restriction. We investigate this possibility directly by regressing non-aid-financed government spending (i.e. total government spending less ODA) on the instrument, including both country and year fixed effects. We find that non-aid-financed government spending is not very significantly correlated with the predicted disbursements instrument: the F-statistic on the instrument is only 7.3 in the full sample, 4.4 in the IDA sample, and 4.5 in the high-aid sample. This suggests that, although other forms of government spending might matter for the exchange rate, their omission from our benchmark specification is unlikely to result in a major violation of the exclusion restriction.

In summary, our benchmark specification suggests a fairly clear pattern. Aid inflows are associated with a small but significant real depreciation in impact. Over time, this gradually turns into small real appreciation that is not significantly different from zero. In the next two sections we subject this benchmark finding to a variety of robustness checks.

## 5. Basic Robustness Checks

In this section of the paper we present a number of straightforward variations on our benchmark specification in order to verify the robustness of these findings. A first potential concern with the benchmark results in the previous section is that they could be driven by a small number of influential observations. This concern is particularly important given the large swings in aid and the real exchange rate documented in Table 1. To verify that our results are not sensitive to a few country-year observations, we use an approach suggested in Hadi (1992) to identify influential observations in the reduced-form and the first-stage regressions (the ratio of the corresponding two slope coefficients at each horizon being the 2SLS estimates of the impulse response). This first basic robustness check is reported in the first panel of Table 3. This procedure identifies 75 (38) (41) influential observations in the full (high aid) (IDA) samples, or only about 3 percent of the total number of observations. Moreover, the estimated impulse responses change only minimally from the benchmark specification when these observations are excluded. The main notable difference is that the strength of the first-stage relationship increases, particularly in the full sample where the first-stage F-statistic increases from 71 to 116.

As a second robustness check, we vary the number of lags of the real effective exchange rate included in the local projections regressions. In the benchmark specification, we included two lags of the real exchange rate, as the first and second lags are consistently significant across all the specifications we considered. In the second panel of Table 3, we report the estimated impulse obtained when a third lag of the real exchange rate is included. Once again, the estimated impulse responses are practically indistinguishable from the benchmark estimates. We note also that the coefficient on the third lag of the real exchange rate is not significantly different from zero, providing some justification for the inclusion of only two lags in our benchmark specification.

We next consider a more conservative version of our identification strategy. Recall that the key identifying assumption underlying the predicted disbursements instrument is that loan approvals in a given year do not anticipate macroeconomic shocks in subsequent years. Conditional on this assumption, predicted disbursements on loans approved in previous years are uncorrelated with contemporaneous macroeconomic shocks. In the third panel of Table 3, we make the weaker assumption that loan approvals may react to macroeconomic shocks in same year and the following year, but not two or more years into the future. We then construct an alternative version of the instrument which excludes predicted disbursements on loans approved in the same and previous year.

Once again, this variation has little effect on the estimated impulse responses, which are very similar to those in the benchmark specification.

In our benchmark results, we use the IMF's trade-weighted multilateral real exchange rate. As a further robustness check, we investigate how our results change if we instead use the more widely-available bilateral real exchange rate, based on nominal exchange rates relative to the US and relative price levels as measured by GDP deflators. The results are shown in the bottom panel of Table 3. While qualitatively the results are similar in this larger sample of observations, quantitatively the main difference is in the contemporaneous effects which are substantially larger than in the benchmark. The estimated impact of a one percent to GDP increase in aid is close to a one percent depreciation in the exchange rate for the full country sample. Over time however, the impulse response becomes closer to the benchmark specification, indicating a small and statistically insignificant real appreciation after 5 years.

Figure 3 provides a visual summary of these first basic robustness checks, in the full sample of countries. It graphs the estimated impulse response and 95 percent confidence intervals in the benchmark specification, and then superimposes the estimated impulse responses for the four robustness checks reported in this section. A quick look at this graph confirms the discussion above -- across these four variations, changes estimated impulse responses relative to the benchmark specification are minimal. In fact, with the exception of the estimated impact effect using the bilateral real exchange rate, all of the estimated points along all impulse responses fall within the 95 percent confidence intervals for the benchmark specification.

## **6. Monetary Policy, Exchange Rate Regimes, and the Composition of Aid-Financed Spending**

We next consider the interaction between monetary policy, the exchange rate regime, and the composition of aid in determining the effect of aid inflows on the real exchange rate. Hussain et. al. (2009) provide a comprehensive discussion of these interactions, which we briefly summarize here in order to motivate several refinements of our basic specification. The two key considerations in determining the effect of aid on the real exchange rate are the extent to which aid resources are absorbed by the recipient economy, and the extent to which aid resources are spent. First, low absorption occurs if central banks use the foreign exchange associated with financial (as opposed to in-kind) aid receipts to accumulate domestic reserves, while high absorption occurs when central banks sell

the foreign exchange to domestic agents. Clearly, upward pressure on the exchange rate will only occur if absorption is high.<sup>5</sup> Second, to the extent that government spending increases in response to the aid inflow, the effect on the real exchange rate will depend on whether it falls on nontradeables or tradeables. For example, if most of the spending falls on imports, upward pressure on the real exchange rate would be limited. However, to the extent that it falls on non-tradeables, and especially those with a low elasticity of supply, it can bid up their relative price, leading to a real appreciation.

While these two mechanisms matter irrespective of the choice of exchange rate regime, the channel of adjustment of the real exchange rate will be different. In the fixed exchange rate regime, the adjustment will occur through increases in the domestic price level, while in a flexible exchange rate regime the adjustment can occur through some combination of prices and the nominal exchange rate. If the nominal exchange rate adjusts faster (slower) than the domestic price level, then the effect of aid on the real exchange rate may occur sooner (later) in a flexible (fixed) exchange rate regime.

We modify our benchmark specifications in three directions in order to be able to assess these considerations. In the first and second panels of Table 4 we estimate the impulse response of the real exchange rate to aid, distinguishing between fixed and flexible exchange rate regimes. We use the definitions and classification as suggested by Ilzetzi et al. (2012) to distinguish country-year observations according to the exchange rate regime.<sup>6</sup> For the fixed exchange rate regime, we find a slightly stronger depreciation on impact than in the benchmark specification, and after five years we find no significant impact on the real exchange rate. Under the flexible exchange rate regime, the initial depreciation is similar in size to the benchmark estimates, but is much less precisely estimated and is not significantly different from zero. After five years, we find a slightly smaller real appreciation than in the benchmark specification. However, as in the benchmark specification, the real appreciation after five years is not significantly different from zero.

In the high-aid and IDA samples, we find a similar pattern that the initial estimated real depreciation is larger on impact in the fixed exchange rate regime when compared with the flexible exchange rate regime. In both country samples, we find no significant effect of aid on the real exchange

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<sup>5</sup> In fact, see Korinek and Serven (2010) for a model in which governments can use reserve accumulation as a tool to undervalue the real exchange rate, in order to benefit from dynamic learning-by-doing effects in the production of tradeables.

<sup>6</sup> We classify the exchange rate regime as fixed where their "coarse" measure is equal to 1 or 2, corresponding to fixed or slowly-crawling pegs, and flexible otherwise.



rate after 5 years. Moreover, the differences between the estimated responses in the two exchange rate regimes are small relative to the estimated standard errors. Overall, the estimated long-run response of the real exchange rate is very similar in the fixed and flexible nominal exchange rate regimes, and even the larger initial depreciation in the flexible exchange rate regime falls within the estimated 95 percent confidence interval for the benchmark estimate.

We next investigate the role of reserve accumulation in potentially obscuring aid-induced real appreciations. Specifically, we augment our baseline specification with the change in net reserves, expressed as a share of GDP. The results do not suggest that controlling for reserve accumulation significantly changes the estimated impact of aid on the real effective exchange rate. We see barely any difference in the size and significance of the estimated impulse response for the full sample. Only in the high aid and IDA samples, we find a contemporaneous depreciation that is slightly smaller than in the benchmark results and less significant.<sup>7</sup>

Finally, we consider the extent to which the composition of aid matters for its impact on the real exchange rate. As discussed above, the most basic version of the theory suggests that aid resources that are spent domestically will have a greater positive effect on the real exchange rate than aid that is spent on imports. Unfortunately, it is difficult to observe this conceptual distinction clearly in the net ODA data with which we work. This is because, while there is considerable detail on different types of aid flows, they are not classified according to the location where they are spent. As a rough approximation, we subtract out from net ODA three categories of aid that are more likely to end up being spent on foreign goods and/or services: technical cooperation (which typically is used to purchase the services of consultants from developed countries); development food aid (which most often is delivered in kind and so does not involve domestic purchases in the recipient country); and humanitarian assistance (which often also is used to purchase goods and services from rich countries that can immediately be used for disaster relief). For the median country-year observation in the full sample, these three categories account for 28% of aid, and for 21% and 23% of aid in the high aid and

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<sup>7</sup> Although not reported for reasons of space, we do find that reserve accumulation is negatively correlated with changes in the real exchange rate, although generally not significantly so. This is consistent with the mechanism discussed above: if capital inflows go to reserve accumulation rather than being spent domestically on non-tradeables, the effect on the real exchange rate will be smaller. Hence, when we control for reserve accumulation we eliminate this source of downward bias in the estimated effect of aid on the real exchange rate. Moreover, the significant result in horizon=3 in the IDA sample is driven by a handful of influential observations. Applying the Hadi procedure leads to a similar result as in the benchmark specification for this sample in this horizon but leaves the coefficient insignificant.

IDA samples. Our prior is that the remaining components of aid are more likely to be spent domestically, and so may be more likely to have a positive effect on the real exchange rate.

The results are shown in the bottom panel of Table 4. Somewhat surprisingly, we find that the estimated impulse responses are nearly identical to those in the benchmark specification, again showing little evidence of a significant positive effect on the real exchange rate, even after 5 years. One interpretation of this finding is that our rough distinction between different types of aid simply is not a good proxy for the extent to which aid is spent domestically. Another interpretation is that, as noted above, even domestically-spent aid need not lead to a real appreciation if it leads to a supply response in the non-traded sector. Unfortunately, absent better data on where aid is spent, we cannot distinguish between these two interpretations.

## **7. Conclusion**

This paper provides new evidence on an old question: do aid inflows have the unintended consequence of leading to a potentially-damaging real appreciation of the exchange rate? Our findings suggest that this should not be a major concern for developing-country policymakers. We find that, on impact, an additional aid inflow of one percentage point of GDP is associated with a small depreciation of about 0.5 percentage points, and this is gradually reversed into a small real appreciation of about 0.5 percentage points after five years. With the exception of the initial real depreciation, these estimated effects are not statistically significantly different from zero. We subject these basic findings to a variety of robustness checks and find that they hold up quite well. We find similar patterns in the response of the real exchange rate to aid when excluding influential observations, when varying the number of lags in our specification, and using alternative versions of the real exchange rate and the instrument.

We also empirically investigated a number of sources of heterogeneity in the relationship between aid and the real exchange rate suggested by theory. We do not find very large differences across exchange rate regimes, nor do we find that over-time variation in reserve accumulation is obscuring the relationship between aid and the real exchange rate regime. We also considered whether the location where aid is spent matters for its effects on the real exchange rate. Perhaps due to our very imperfect proxy for aid spent abroad, we do not find much difference along this dimension either in terms of effects on the real exchange rate.

From a methodological standpoint, this paper has offered two contributions beyond these immediate findings. First, unlike the majority of empirical papers in this literature, we have suggested

an identification strategy that isolates a plausibly predetermined source of fluctuations in aid, that can help to isolate a causal effect of aid on the real exchange rate. Second, we rely on local projections as suggested by Jordà (2005) to more flexibly characterize the dynamic response of aid to the real exchange rate.

Although the findings here suggest at best a modest impact of aid on the real exchange rate, a useful caveat is that we have estimated only average responses within fairly large country groups. It is possible that there is significant heterogeneity in country-level responses of the real exchange rate to aid. It remains an open question for further research whether these country-level differences can be identified with the tools suggested here, and given the limited -- and often noisy -- time series coverage of data on aid and real exchange rates for many of the countries in our sample.

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**Table 1: Summary statistics**

	<b>N</b>	<b>Std.Dev</b>	<b>Real effective exchange rate (log)</b>	<b>Net ODA/ GDP</b>	<b>Predicted disbursements/ GDP</b>
<b>Full Sample</b>					
Real effective exchange rate (log)	2418	0.11	1.00		
Netoda/GDP	2418	0.04	-0.09	1.00	
Predicted disbursements/GDP	2418	0.01	-0.07	0.39	1.00
<b>High Disbursement Sample</b>					
Real effective exchange rate (log)	1212	0.11	1.00		
Netoda/GDP	1212	0.06	-0.13	1.00	
Predicted disbursements	1212	0.01	-0.10	0.37	1.00
<b>IDA Sample</b>					
Real effective exchange rate (log)	1274	0.12	1.00		
Netoda/GDP	1274	0.05	-0.11	1.00	
Predicted disbursements	1274	0.01	-0.08	0.39	1.00

Notes: This table reports summary statistics on the indicated variables. Aid and predicted disbursements are expressed as a share of GDP, and the real exchange rate is in logarithms. All variables are in terms of deviations from country- and year-averages, consistent with the inclusion of country and year fixed effects in Equation (1).

**Table 2: Benchmark specification**

Sample of countries		<u>Full</u>		<u>High Aid</u>		<u>IDA</u>	
<b>Panel A: OLS Estimates</b>							
Dependent variable is the log of the trade-weighted real exchange rate at horizon h							
Estimated impact on the real exchange rate at horizon h, of aid (Net ODA) as a share of GDP at horizon h:	<b>h=0</b>	-0.233***	(0.075)	-0.256***	(0.084)	-0.242***	(0.083)
	<b>h=1</b>	-0.221*	(0.117)	-0.242*	(0.125)	-0.211*	(0.121)
	<b>h=2</b>	-0.219	(0.145)	-0.234	(0.153)	-0.198	(0.148)
	<b>h=3</b>	-0.206	(0.161)	-0.177	(0.155)	-0.150	(0.157)
	<b>h=4</b>	-0.157	(0.175)	-0.086	(0.158)	-0.087	(0.171)
	<b>h=5</b>	-0.090	(0.197)	0.028	(0.179)	0.045	(0.195)
Lagged rxr		0.985***	(0.039)	0.921***	(0.058)	0.944***	(0.050)
Twice lagged rxr		-0.158***	(0.030)	-0.135***	(0.044)	-0.145***	(0.038)
<b>Panel B: Instrumental Variable Estimates</b>							
Dependent variable is the log of the trade-weighted real exchange rate at horizon h							
Estimated impact on the real exchange rate at horizon h, of predicted disbursements as a share of GDP at horizon h:	<b>h=0</b>	-0.487***	(0.184)	-0.547***	(0.189)	-0.487***	(0.177)
	<b>h=1</b>	-0.113	(0.262)	-0.247	(0.234)	-0.116	(0.237)
	<b>h=2</b>	0.154	(0.388)	-0.016	(0.372)	0.133	(0.376)
	<b>h=3</b>	0.358	(0.465)	0.213	(0.435)	0.399	(0.422)
	<b>h=4</b>	0.513	(0.508)	0.380	(0.479)	0.547	(0.461)
	<b>h=5</b>	0.666	(0.556)	0.553	(0.516)	0.718	(0.480)
Lagged rxr		0.976***	(0.040)	0.901***	(0.062)	0.933***	(0.052)
Twice lagged rxr		-0.158***	(0.030)	-0.133***	(0.044)	-0.143***	(0.038)
First-stage F-Statistic on excluded instrument		70.88		48.64		57.32	
No. of Obs.	<b>h=0</b>	2418		1212		1274	
No. of Countries	<b>h=0</b>	104		53		58	

Note: \*\* (\*\*) (\*) denotes significance at the 1, 5, and 10 percent level. Heteroskedasticity-consistent standard errors are clustered at the country level. Net ODA and the predicted disbursements instrument are both scaled by GDP. Panels A and B report OLS and instrumental variable estimates of Equation (1), including a full set of country and year fixed effects. The three columns correspond to the full sample of countries, the high net ODA recipient sample, and the sample of IDA-eligible countries. The sample of high net ODA recipients consists of countries with net aid exceeding 6 percent of GDP averaged over the sample period. Coefficients on lags of the dependent variable, and number of observations, correspond to horizon h=0 of the local projection regression.

**Table 3: Robustness Checks – Part I**

Instrumental Variable Estimates only							
Sample of countries		Full		High Aid		IDA	
Excluding Influential Observations							
Estimated impact on the real exchange rate at horizon h, of predicted disbursements as a share of GDP at	h=0	-0.485***	(0.155)	-0.387***	(0.132)	-0.463***	(0.134)
	h=1	-0.215	(0.272)	-0.304	(0.247)	-0.129	(0.250)
	h=2	0.163	(0.414)	0.033	(0.326)	0.174	(0.372)
	h=3	0.221	(0.484)	0.064	(0.403)	0.167	(0.430)
	h=4	0.250	(0.539)	0.071	(0.433)	0.291	(0.451)
	h=5	0.488	(0.565)	0.318	(0.471)	0.498	(0.466)
FSR F-Statistic	h=0	115.78		55.96		64.37	
No. of Obs.	h=0	2343		1174		1233	
Adding a Third Lag of RXR							
Estimated impact on the real exchange rate at horizon h, of predicted disbursements as a share of GDP at	h=0	-0.563***	(0.171)	-0.595***	(0.170)	-0.547***	(0.156)
	h=1	-0.229	(0.265)	-0.397	(0.244)	-0.261	(0.248)
	h=2	0.047	(0.364)	-0.124	(0.331)	0.019	(0.346)
	h=3	0.211	(0.436)	0.041	(0.385)	0.225	(0.389)
	h=4	0.379	(0.477)	0.202	(0.417)	0.377	(0.412)
	h=5	0.524	(0.523)	0.333	(0.465)	0.531	(0.440)
FSR F-Statistic		76.61		48.22		56.54	
No. of Obs.	h=0	2313		1156		1212	
Using a More Conservative Predicted Disbursements Instrument							
on the real exchange rate at horizon h, of aid (Net ODA) as a share of GDP at horizon h	h=0	-0.579***	(0.164)	-0.574***	(0.153)	-0.524***	(0.158)
	h=1	-0.225	(0.251)	-0.259	(0.253)	-0.093	(0.258)
	h=2	0.016	(0.374)	0.010	(0.417)	0.204	(0.414)
	h=3	0.148	(0.439)	0.156	(0.484)	0.412	(0.456)
	h=4	0.213	(0.493)	0.205	(0.539)	0.474	(0.505)
	h=5	0.357	(0.532)	0.374	(0.561)	0.655	(0.510)
FSR F-Statistic		81.14		55.33		56.46	
No. of Obs.		2409		1203		1265	
Using a Bilateral RXR Measure							
Estimated impact on the real exchange rate at horizon h, of predicted disbursements as a	h=0	-0.945***	(0.174)	-0.936***	(0.208)	-0.930***	(0.199)
	h=1	-0.597***	(0.226)	-0.539**	(0.269)	-0.553*	(0.290)
	h=2	-0.270	(0.292)	-0.131	(0.365)	-0.142	(0.375)
	h=3	-0.153	(0.326)	0.057	(0.414)	0.038	(0.409)
	h=4	0.032	(0.336)	0.235	(0.399)	0.202	(0.380)
	h=5	0.150	(0.364)	0.377	(0.406)	0.402	(0.402)
FSR F-Statistic		121.18		64.74		93.94	
No. of Obs.	h=0	3472		1734		1844	

Note: \*\* (\*\*\*) (\*) denotes significance at the 1, 5, and 10 percent level. Heteroskedasticity-consistent standard errors are clustered at the country level. Table 3 reports 2SLS estimates of local projection regressions in Equation (1). Coefficients on lags of the dependent variable are not reported to conserve space. Net ODA and the predicted disbursements instrument are both scaled by GDP. Panels A and B report OLS and instrumental variable estimates of Equation (1), including a full set of country and year fixed effects. The three columns correspond to the full sample of countries, the high net ODA recipient sample, and the sample of IDA-eligible countries. The sample of high net ODA recipients consists of countries with net aid exceeding 6 percent of GDP averaged over the sample period. Coefficients on lags of the dependent variable, and number of observations, correspond to horizon h=0 of the local projection regression.

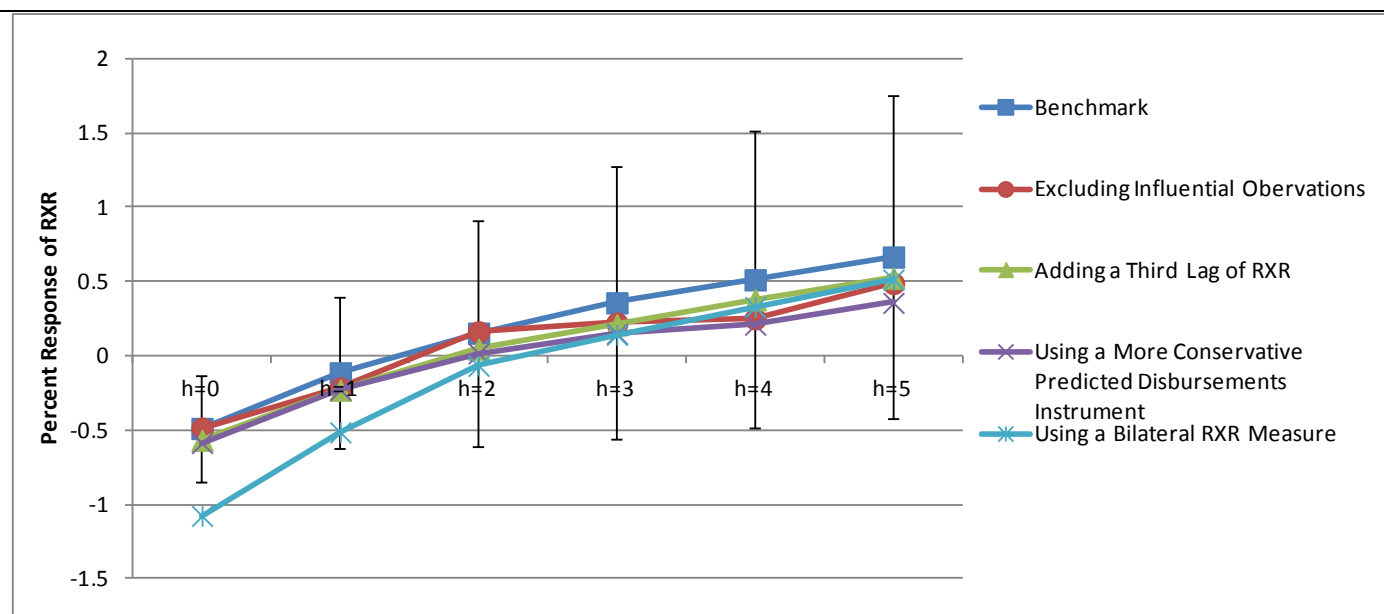


**Table 4: Robustness Checks – Part II**

Sample of countries		Full		High Aid		IDA	
<b>Fixed Exchange Rate Regime Sample</b>							
Estimated impact on the real exchange rate at horizon h, of aid (Net ODA) as a share of GDP at horizon h	h=0	-0.809***	(0.246)	-0.780**	(0.304)	-0.614**	(0.259)
	h=1	-0.672*	(0.365)	-0.542	(0.426)	-0.336	(0.392)
	h=2	-0.489	(0.515)	-0.248	(0.589)	-0.094	(0.514)
	h=3	-0.364	(0.731)	0.095	(0.813)	0.158	(0.684)
	h=4	-0.085	(0.890)	0.535	(0.999)	0.433	(0.838)
	h=5	-0.014	(0.996)	0.669	(1.044)	0.417	(0.880)
FSR F-Statistic		31.33		18.7		25.36	
No. of Obs.		h=0	1522	784		862	
<b>Flexible Exchange Rate Regime Sample</b>							
Estimated impact on the real exchange rate at horizon h, of aid (Net ODA) as a share of GDP at horizon h	h=0	-0.496	(0.346)	-0.440	(0.315)	-0.487	(0.419)
	h=1	-0.098	(0.400)	-0.254	(0.374)	-0.089	(0.479)
	h=2	0.094	(0.535)	-0.094	(0.474)	0.085	(0.597)
	h=3	0.125	(0.618)	-0.096	(0.476)	0.130	(0.561)
	h=4	0.121	(0.670)	-0.049	(0.514)	0.217	(0.563)
	h=5	0.344	(0.727)	0.163	(0.599)	0.655	(0.586)
FSR F-Statistic		32.25		33.77		25.69	
No. of Obs.		h=0	892	424		408	
<b>Controlling for Changes in Net Reserves</b>							
Estimated impact on the real exchange rate at horizon h, of aid (Net ODA) as a share of GDP at horizon h	h=0	-0.469**	(0.192)	-0.424**	(0.175)	-0.325*	(0.166)
	h=1	0.051	(0.307)	-0.027	(0.268)	0.190	(0.280)
	h=2	0.452	(0.457)	0.352	(0.428)	0.574	(0.437)
	h=3	0.689	(0.575)	0.633	(0.517)	0.853*	(0.509)
	h=4	0.647	(0.622)	0.576	(0.553)	0.647	(0.545)
	h=5	0.762	(0.685)	0.768	(0.613)	0.810	(0.600)
FSR F-Statistic		56.63		36.81		44.5	
No. of Obs.		h=0	2212	1077		1122	
<b>Excluding Technical Assistance, Humanitarian Assistance, Development Food Aid</b>							
Estimated impact on the real exchange rate at horizon h, of aid (Net ODA) as a share of GDP at horizon h	h=0	-0.692***	(0.261)	-0.791***	(0.272)	-0.715***	(0.252)
	h=1	-0.160	(0.371)	-0.357	(0.336)	-0.170	(0.347)
	h=2	0.216	(0.547)	-0.023	(0.537)	0.193	(0.546)
	h=3	0.499	(0.652)	0.308	(0.633)	0.575	(0.611)
	h=4	0.707	(0.708)	0.548	(0.698)	0.784	(0.667)
	h=5	0.910	(0.770)	0.790	(0.750)	1.020	(0.686)
FSR F-Statistic		71.22		76.78		53.5	
No. of Obs.		h=0	2418	1212		1274	

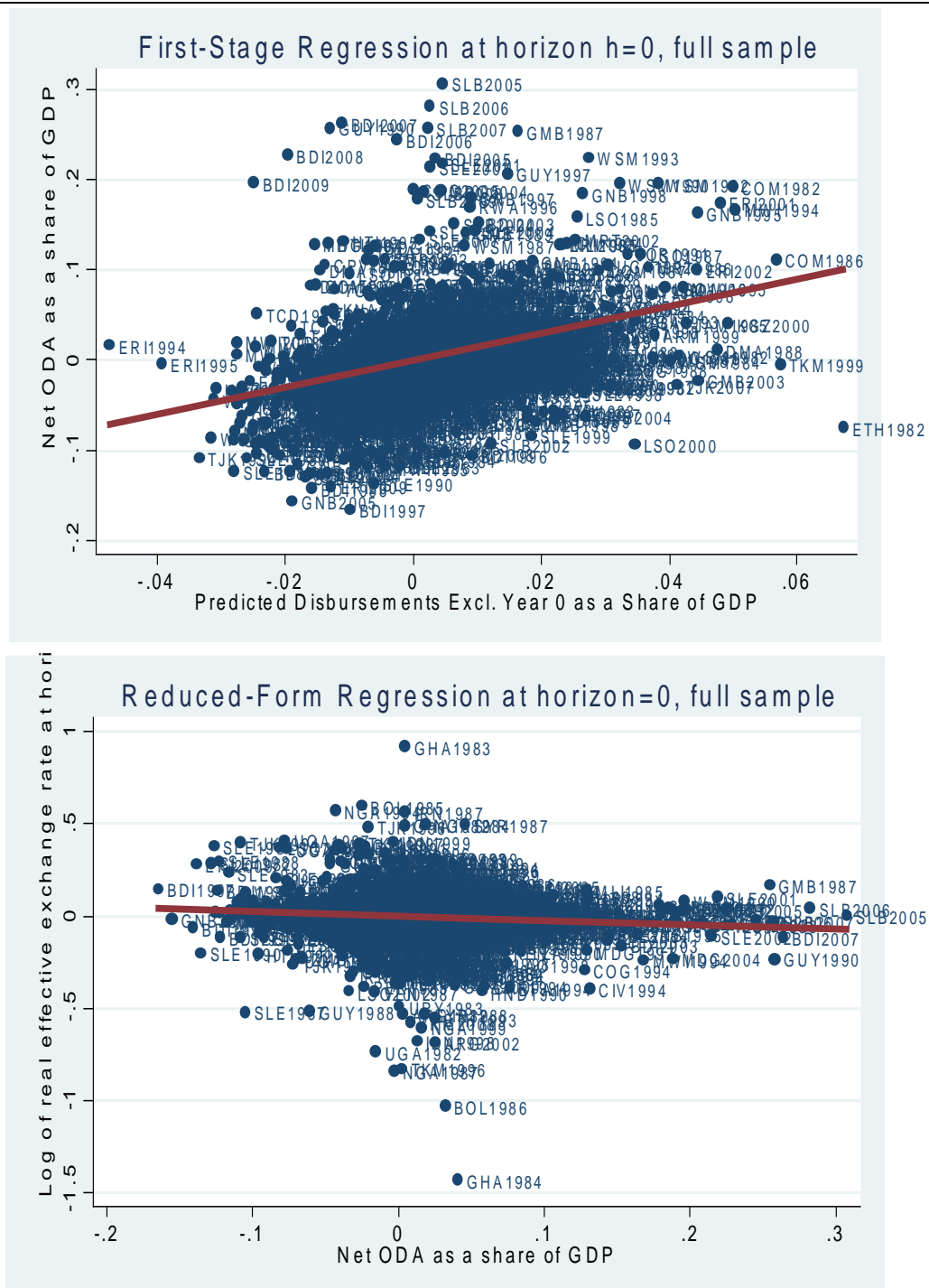
Note: \*\* (\*\*\*) (\*) denotes significance at the 1, 5, and 10 percent level. Heteroskedasticity-consistent standard errors are clustered at the country level. Table 4 reports 2SLS estimates of local projection regressions in Equation (1). Coefficients on lags of the dependent variable are not reported to conserve space. Net ODA and the predicted disbursements instrument are both scaled by GDP. Panels A and B report OLS and instrumental variable estimates of Equation (1), including a full set of country and year fixed effects. The three columns correspond to the full sample of countries, the high net ODA recipient sample, and the sample of IDA-eligible countries. The sample of high net ODA recipients consists of countries with net aid exceeding 6 percent of GDP averaged over the sample period. Coefficients on lags of the dependent variable, and number of observations, correspond to horizon h=0 of the local projection regression.

**Figure 1: Estimates of Impulse Response of Real Exchange Rate to Aid, Benchmark Specification**



Notes: This figure shows 2SLS estimates of the percent response of the real exchange rate to a one percentage point increase in aid as a fraction of GDP, for  $h=0, \dots, 5$  years following the increase in aid. 95% confidence intervals based on heteroskedasticity-consistent standard errors clustered at the country level are shown as vertical lines.

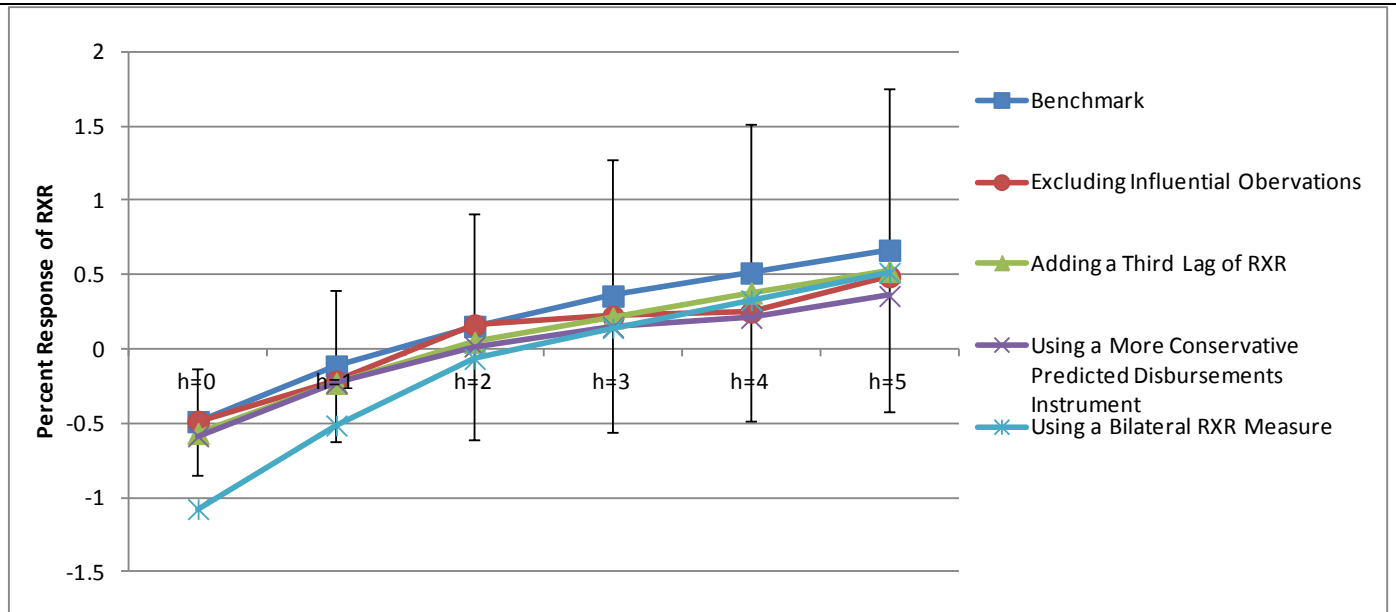
**Figure 2: First-Stage (Top Panel) and Reduced-Form (Bottom Panel) Regressions**



Notes: The top panel shows the estimated first-stage relationship between net ODA and predicted disbursements on loans from official creditors. Both variables are scaled by GDP and expressed as deviations from country- and year-specific means. The bottom panel shows the reduced-form relationship between the logarithm of the real exchange rate and predicted disbursements as a fraction of GDP, also in deviations from country- and year-specific means.

**Figure 3: Estimated Impulse Responses**

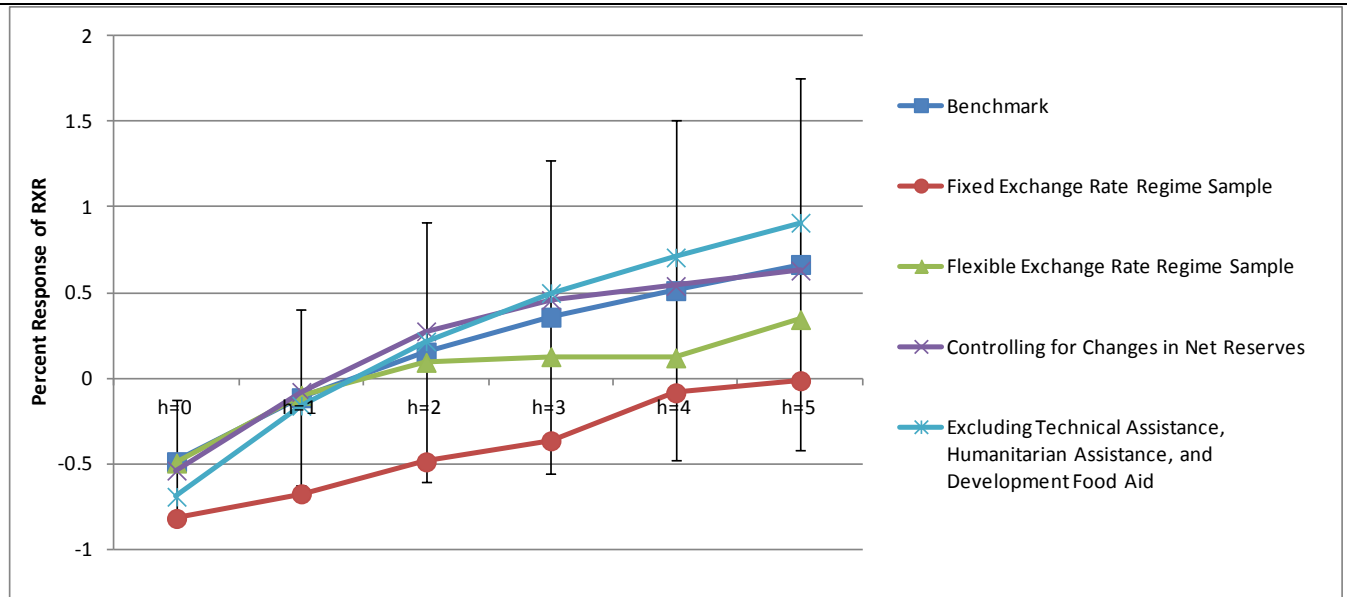
**(Table 3, Full Sample)**



Notes: All graphs display 2SLS estimates of the impulse response of the real exchange rate to a one percentage point of GDP increase in aid over five years. The 95% confidence intervals are shown around the benchmark specification only. The four additional reported impulse responses correspond to the full sample estimates reported in Table 3.

**Figure 4: Estimated Impulse Responses**

**(Table 4, Full Sample)**



Notes: All graphs display 2SLS estimates of the impulse response of the real exchange rate to a one percentage point of GDP increase in aid over five years. The 95% confidence intervals are shown around the benchmark specification only. The four additional reported impulse responses correspond to the full sample estimates reported in Table 4.